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XXVII.—*On the Modern Philosophical Conceptions of Life.* By J. J. Woodward, President of the Philosophical Society of Washington *.

I propose to invite your attention this evening to some thoughts on the Modern Philosophical Conceptions of Life. The theme is so large that it would be idle to attempt its systematic treatment in the course of a single evening; nor do I pretend to be in possession of any satisfactory solution of this ancient question, of which I might offer you an abstract or outline, pending the fuller presentation of my results elsewhere. Yet I have ventured to hope that a discussion of some of the considerations involved, and a brief statement of certain views that I have been led to entertain, would not be without interest, and perhaps might prove of actual service, especially to those of you who are engaged in biological pursuits.

Undoubtedly the conception of life most popular at the present time is that which assumes all the phenomena of living beings to be the necessary results of the chemical and physical forces of the universe, and claims or intimates that wherever this has not yet been proven to be the case the evidence will hereafter be forthcoming. This doctrine, which

* From the 'Bulletin of the Philosophical Society, Washington,' 1883.

may conveniently be designated the chemico-physical hypothesis of life, has readily found its way from the speculative writings of philosophers to the rostrums of some of our teachers of chemistry and physics, who boldly declare, in their class-lectures and public addresses, that the forces at work in the inorganic world are fully adequate to explain all the phenomena of living beings, and prophesy that the time is soon coming "when the last vestige of the vital principle as an independent entity shall disappear from the terminology of science" *.

Now most of these gentlemen are not embarrassed by any very definite or detailed knowledge of the physiological and pathological phenomena which a tenable theory of life must be competent to explain, while they do know, or at least ought to know, a great deal of chemistry and physics; the confidence with which they maintain their creed is therefore readily understood. Much more surprising is it to find the same doctrine embraced by numerous zoologists, physiologists, nay, even pathologists, among them men who cannot for a moment be supposed to be unacquainted with the phenomena to be explained, and of whose abilities and reasoning powers it is impossible for me to think or speak otherwise than respectfully. Yet I cannot but believe that they have adopted the chemico-physical hypothesis, not so much because they are really satisfied with it as a scientific explanation of all the phenomena, as because they are unduly biassed in its favour by the utterances of the great philosopher who has done, as I think we will all agree, such good service to biological science by elaborating and popularizing the doctrine of evolution.

It is only natural that such a bias should exist. The discussion of the nature of life, in the case of man at least, has always, and not unreasonably, been conjoined with the discussion of the nature of the soul; and the philosophers who have won higher repute in the latter discussion have always been willing enough to offer solutions of the life-problem, and have never had any difficulty in finding followers even among those whose special lines of investigation might be supposed to impose upon them the duty of independent inquiry into the meaning of life.

Just as it was in the old time with regard to this matter, so it is now. When Galen undertakes to discuss the complex

* George F. Barker, "Some Modern Aspects of the Life Question" (Address as President of the Amer. Assoc. for the Advancement of Science, Boston meeting, August 1880; 'Proceedings,' vol. xxix. part i. p. 23).

phenomena of the Psyche, as manifested by the human species, he openly and continually confesses the extent to which he relies upon the authority of Plato; and when the dicta of the master are such as to require a special effort of faith on the part of the disciple, he honestly exclaims, "Plato indeed appears to be persuaded of this; as for me, whether it be so or not, I am unable to dispute the question with him" *.

In like manner, did they venture to be as frank as Galen was, most of the modern biologists who have adopted the chemico-physical theory of life would, I presume, confess, "As to this matter our opinions are derived from Mr. Herbert Spencer's 'Principles of Biology;' what are we that we should venture to dispute as to questions like these with him?"

Nevertheless in striking contrast to this chemico-physical hypothesis of life, which is to be regarded as the fashionable faith of the hour, there still survives in many quarters, and especially among physicians, a disposition to regard indiscriminately almost all the phenomena of living beings as peculiar manifestations of a vital principle. So strong, indeed, is the faith of some of these modern vitalists, that they seem to shut their eyes to the evidence already in our possession as to the actual participation of known chemical and physical forces in the operations going on within living bodies, and appear almost to resent the willing aid that chemistry and physics afford to the physiological investigator of the present day.

Nay, further than this, in the inevitable reaction that is beginning to make itself felt against the avowed revival of the materialism of Epicurus and Lucretius—for we all know now that the chemico-physical hypothesis of life is not a new induction of modern science, but an ancient Greek speculation reappearing in modern petticoats—that other Greek speculation of the threefold Psyche, the doctrine taught by Plato and Aristotle, and which Galen accepted on their authority, the doctrine of a vegetable, an animal, and a rational soul, a human trinity coexisting in every human being, is once more rehabilitated and finding followers—likely, indeed, as I think, to obtain more followers than perhaps any of you yet suppose. And these followers are by no means confined to metaphysicians or churchmen; they can be found also already among the biologists. It is an English biologist of good repute and of no mean abilities who takes occasion, in a technical biological work published this very year, to express his belief that

* Galen, 'Quod animi mores corporis temperamenta sequantur,' cap. 3 (Kühn's edit. t. iv. p. 772).

the Greek conception of the threefold Psyche "appears to be justified by the light of the science of our own day" *.

For myself I must confess at once that I am quite unable to join either of these opposing camps as a partisan. I cannot accept the more strictly vitalistic views, because I am compelled continually to recognize the operation of purely chemical and physical forces in living beings. On the other hand, there are whole groups of phenomena characteristic of living beings and peculiar to them of which the chemico-physical hypothesis offers no intelligible explanation.

From this point of view the various processes and functions of living beings may indeed be divided into two classes, of which the first may be regarded with more or less certainty as the special results, under special conditions, of the very same forces that operate in the inorganic world; while the second, to which alone I would apply the term vital, are not merely in every respect peculiar to living beings, and hitherto utterly inexplicable by the laws of chemistry and physics, but are so different in character from the phenomena of the inorganic world, that it does not seem rational to attempt to explain them by these laws.

Let me refer briefly to the processes and functions belonging to the first class. Here I place all those more strictly chemical processes by which, within the very substance of vegetable protoplasm, inorganic elements are combined into organic matter, as well as those which produce all the various subsequent transformations, whether in plants or animals, of the organic matter thus prepared. This general conception includes of course, in the case of the higher animals, all the chemical phases of the processes of digestion, assimilation, and tissue-metamorphosis or metabolism, including secretion and excretion; in the case of the lower animals and plants, so much of these several functions as belongs to each species.

Now please to understand that when I say I recognize all the chemical phases of these processes to be the results of the ordinary chemical laws, I do not entertain any mental reservation with regard to the unrestricted application of these laws. I cannot for a moment agree with those physiologists who have imagined the vital principle to thwart or interfere with or counteract these laws in any way. I know indeed that we are far from being as thoroughly acquainted as we may by and by hope to be with the chemical phenomena of living beings; that many of the questions are very difficult, so that as yet, with all our labour, we have obtained but partial or even contradictory results; but I find in this only a reason for further

* St. George Mivart, 'The Cat' (London, 1881), p. 387.

investigation—no logical difficulty of a radical kind. In a general way I recognize that the matter of which living beings are composed is built up of elementary substances belonging to the inorganic world, and that it consists of atoms possessed of the very same properties and obedient to the very same laws as like atoms in inorganic bodies. Yet I confess I find in all this no reason for denying the existence of a vital principle; only I do not figure this principle in my mind as a hostile power interfering in any way with the chemical tendencies of the atoms present; I liken its operations rather to those of the chemist in his laboratory who obtains the results he needs only on the condition of most rigid obedience to chemical laws.

Intimately associated with some of the chemical processes just enumerated are those chemical processes of respiration in which the chemical affinities of the oxygen of the atmosphere are directly or indirectly the means of promoting tissue metamorphosis, as well as of reducing at once to simpler forms some portion of the various complex substances derived from the food. These chemical processes are undoubtedly the chief original sources of the heat and mechanical power manifested by animals. Of course they receive heat also from without by conduction and radiation; but this is a small matter to the heat generated within them; of course, too, mechanical power is continually transformed into heat within the body of animals; but this neither increases nor diminishes the total amount of energy liberated.

I yield my hearty assent to that modern scientific induction * which sees in the potential energy of the complex chemical compounds supplied to animals by their food the essential source of all the actual energy of the body, whether manifested in the form of heat or work. In a general way the reduction of these complex chemical compounds by oxidation into the much simpler ones, urea, carbon dioxide, and water, is the means by which potential is converted into actual energy. In the case of plants, too, the source of any little heat that may be developed under special conditions, and of such sluggish motions as actually occur, is doubtless to be found in the reduction to simpler combinations by oxidation of a part of the organic matter already formed. The chief function of the vegetable world, however, is to build up, by means of the solar energy, those complex and unstable

* First taught by J. R. Mayer, 'Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: Ein Beitrag zur Naturkunde' (Heilbronn, 1845).

organic compounds that supply the animal world with food. Nevertheless, while I yield my hearty assent to this generalization, and freely admit that it is more than a mere deduction from the general doctrine of the conservation of energy—that, in fact, it affords the most satisfactory explanation yet suggested for a large number of observed phenomena—it is my duty to caution you against the erroneous supposition that any one has ever yet succeeded in affording a rigorous demonstration of the truth of the generalization by an adequate series of actual experiments.

Various attempts have indeed been made of late years to determine experimentally both for animals and for man the potential energy contained in the food of a given period, and the actual energy liberated during the same time in the form of heat and work. I think, however, that all practical physiologists who have looked into the question will agree with me that the numerical results hitherto obtained must be received with the utmost caution *. Difficulties exist on both sides of the problem. It is comparatively easy no doubt to obtain a close approximation to the quantity and composition of the food; but to represent numerically what becomes of it in the body, to deduct correctly what passes through unchanged, and ascertain with reasonable accuracy the amount of carbon dioxide, water, and urea into which the rest is transformed, these are questions which have taxed the utmost resources of investigators, and as to which our knowledge is yet in its infancy.

On the other hand, the direct measurement of the resulting heat and work has hitherto proved still less satisfactory. It would seem to be a very simple thing to place an animal in a calorimeter and measure the heat-units evolved in a given time, as Lavoisier and Laplace attempted to do in the latter part of the last century; and we have been told that "Lavoisier's guinea-pig placed in the calorimeter gave as accurate a return for the energy it had absorbed in its food as any thermic engine would have done" †. But this assertion is not supported by the results of actual experiment. We know now that many precautions, unknown to Lavoisier, must be taken to secure any approach to accuracy in calorimetric experiments with animals; and just as the method is being brought to something like perfection, by arranging for the respiratory process and its influence on the results, and by other neces-

* See, for example, M. Foster, 'Text-book of Physiology' (2nd edit. London, 1878, p. 355).

† Barker, *op. cit. suprà*

sary modifications of the primitive rude attempts *, doubts are beginning to arise as to whether after all the conditions in which the animal is placed in the calorimeter are not so far abnormal as seriously to vitiate the results †; so that, in fact, the most approved numerical expressions of the heat-production of the body to be found in the books are based rather upon calculation of the amount that ought to be produced by the oxidation of an estimated quantity of food than upon actual calorimetric observations.

Nor do we find it any easier when we attempt the actual measurement of the amount of work produced by an animal from a given amount of food. Indeed, in attempting to formulate an equation between the potential energy of the food and the actual amount of heat and work in any given case, we are met with the special difficulty that the animal does not evolve less heat because it is doing work than it does when it is at rest; on the contrary, it actually evolves more heat, consuming for the purpose more food than usual, or, if this is not forthcoming, consuming a part of its own reserve of adipose tissue; so that from this source fresh complications of the problem arise.

The labour and ingenuity with which all these difficulties have been encountered is certainly worthy of the highest praise, and I willingly admit the probably approximate truth of the figures generally in use, say $2\frac{1}{4}$ to $2\frac{3}{4}$ million gramme-degrees as the daily average heat-production of an adult man, and 150,000 to 200,000 metre-kilogrammes as his capacity for daily mechanical work ‡. Nevertheless, these figures are after all only probable approximations, and there still exists, with regard to these questions, a large and inviting field for the application of chemical and physical methods to physiological research.

All the mechanical work done by living beings is effected by means of certain contractions of their soft tissues. The movements of the Amœba, so often described of late years, may be taken as the type of the simplest form of these contractions. Similar movements occur, with more or less activity, in the protoplasm of all young cells, and in the higher animals are strikingly illustrated by the movements of the white corpuscles of the blood and the wandering cells of the connective tissue. In the lowest animal forms these

* See H. Senator, "Unters. über die Wärmebildung und den Stoffwechsel," Archiv für Anat. Phys. und wiss. Med. 1872, S. 1.

† Foster, p. 368, *op. cit. suprà.*

‡ L. Landois, Lehrb. der Phys. des Menschen (Vienna, 1879), S. 402.

simple amœboid movements of the protoplasm are the only movements; but in the higher forms, besides these, certain special contractile tissues make their appearance, by which the chief part of the mechanical work done is effected; these are the striated and unstriated muscular fibres.

On account of the extreme minuteness of the little protoplasmic bodies in which the amœboid movements are manifested, the investigation of the mechanical means by which these movements are effected has not as yet been attempted, although a great mass of details have been accumulated by actual observation with regard to the phenomena themselves and the conditions under which they occur. Very little more has been done with regard to the contractions of the unstriated muscular fibres. The striated muscles, however, have been made the subject of a host of researches; and I suppose the conclusions to which we may ultimately be led by these can be regarded, with but little reservation, as applicable to the function of the unstriated muscles, and also to the simpler amœboid protoplasmic contractions.

Yet, notwithstanding the vast amount of experimental labour and speculative ingenuity that has been lavished since the time of Haller upon the question of the contraction of the striated muscle, it must be confessed, in the honest language of Hermann *, that the problem still mocks our best endeavours. For myself, I am unwilling to believe that the phenomena of muscular contraction, or, indeed, of any of the varieties of protoplasmic contraction by which animals effect mechanical work, will not by and by be fully and satisfactorily explained on chemico-physical principles. I cannot for a moment give my adherence to the dogmatism of those modern vitalists who insist that the contractions of a muscle or of an Amœba are essentially vital phenomena; for this would be to claim that life can create force. But it would be folly to shut our eyes to the circumstance that no chemico-physical explanation of muscular contraction yet offered has been so convincingly supported by facts as to command the universal assent of competent physiologists.

Of the various hypotheses devised to explain muscular contraction, those which regard the phenomena as in some way resulting from electrical disturbances have long enjoyed great popularity. Such of these hypotheses as still survive are based upon the electrical manifestations actually observed in living muscles. It has been pretty generally accepted in accordance with the observations of Du Bois-Reymond, whose

* L. Hermann, Handb. der Phys. Bd. i. Th. 1, S. 242.

brilliant series of experiments in animal electricity * is deservedly renowned, that even quiescent living muscles are in a state of electrical tension. If, for example, a muscle composed of parallel longitudinal fibres be exposed with suitable precautions, and divided near each extremity by a transverse incision, the surface of the muscle will be found to be positive to the cut ends, and if one of a pair of non-polarizable electrodes, connected with a suitable galvanometer, is placed in contact with the surface of the muscle and the other in contact with one of the cut ends, the existence of a current is made manifest. The conditions are, moreover, such that while the maximum effect is produced when the equator of the surface is connected with the centre of one of the cut ends, more or less current will also be manifested whenever any two points of the surface are thus connected with the galvanometer, provided they are not equidistant from the equator. In such cases the point most distant from the equator is always negative. The electromotive force of this natural current of the quiescent muscle varies greatly, but has been found by Du Bois-Reymond to amount sometimes to as much as ·08 Daniell in one of the thigh-muscles of the frog †. In muscles of different form or cut differently from what has just been described the currents are somewhat differently arranged; but the example just given must suffice for my present purpose.

In accordance with the observations of the same investigator, it is claimed that during a muscular contraction the electrical tension diminishes, the normal muscle-current experiences a negative variation, and this occurs in such a way that, as the wave of actual contraction moves along the muscle, which it does, according to the observations of Bernstein and Hermann ‡, with a velocity of about 3 metres per second, it is preceded by a wave of negative variation. This negative variation is indeed so trifling if the muscle contracts but once, that it is difficult to observe it; but when the contractions succeed each other with great rapidity, as in artificially produced tetanus, it may become sufficient to neutralize completely the deflection of the galvanometer due to the current of the quiescent muscle.

But the belief that the electrical currents shown to exist in the

* Emil Du Bois-Reymond, 'Unters. über thierische Elektricität' (Berlin, 1848–60), and 'Gesammelte Abhandl. zur allgemeinen Muskel- und Nervenphysik' (Leipsic, 1875–77).

† Du Bois-Reymond, Ges. Abhandl. Bd. ii. S. 243.

‡ Bernstein, 'Unters. über den Erregungsvorgang im Nerven- und Muskelsysteme' (Heidelberg, 1871); also Du Bois-Reymond's 'Archiv,' 1875, S. 526; Hermann, in Pflüger's 'Archiv,' Bd. x. 1875, S. 48.

quiescent muscles in these experiments exist also in uninjured animals has not remained unchallenged. Since 1867 it has been attacked especially by Hermann*, who has endeavoured to show that these currents are produced only under the special conditions of the experiments, and that there are in reality no natural muscle-currents at all. It was well known that the currents observed in the experiments varied greatly under different circumstances, and it seemed a significant fact that they should be most intense when the muscle was removed from the body and had both ends cut off. If the muscle was removed with its tendinous extremities still attached, the current was usually found to be very feeble or entirely absent, until the ends were well washed in salt and water or dipped in acid. Du Bois-Reymond had explained this by supposing the natural ends of the muscle to be protected by what he called a parelectronomic layer of positive elements that must be removed before the natural current could be made manifest. On the other hand, Hermann has endeavoured to show that the parts injured by the knife or acted on by the salt or acid enter at once into the well-known condition of rigor mortis, and only become negative to the still living portions of the muscle in consequence of this change. That electrical disturbances actually occur in contracting muscles he admits, but endeavours to show that they are due simply to the fact that the changes preceding contraction make the affected part of the muscle negative to every part less modified or wholly unaltered. Hence if an uninjured muscle be caused, under proper precautions, to contract simultaneously in all its parts, it will be found that the contraction is wholly unaccompanied by any muscle-current †.

Observations that appear to support these views of Hermann have been brought forward by Engelmann ‡. On the other hand, Du Bois-Reymond has defended his views with vigour, and sharply criticized, of course, the labours and logic of his assailant §. I need not at present express any opinion as to the merits of this voluminous controversy. It is enough for my purpose to indicate the questions at issue as sufficiently important and uncertain to be well worthy of independent experimental criticism.

Suppose, however, this criticism should result in showing

* L. Hermann, 'Weitere Unters. zur Phys. der Muskeln und Nerven' (Berlin, 1867); also Handb. der Phys. Bd. i. Th. 1 (Leipsic, 1879), S. 192 *et seq.*

† Hermann, Handb. der Phys. Bd. i. Th. 1, S. 215.

‡ Engelmann, Pflüger's 'Archiv,' Bd. xv. (1877), S. 116 *et seq.*

§ Du Bois-Reymond, Ges. Abhandl. Bd. ii. S. 319 *et seq.*

that Hermann is wholly in the wrong and that the muscle-currents observed by Du Bois-Reymond really exist in healthy muscles. How then shall these currents explain the phenomena of muscular contraction? I presume that no physiologist of the present day is misled by the superficial comparison which Mayer and Amici were led by their microscopical studies of the muscles of insects to make between the striated muscular fibre and a voltaic pile *. But the molecular theory by which Du Bois-Reymond has endeavoured to explain his natural muscle-currents and their negative variation would appear to open up an inexhaustible mine of speculative possibilities for those who are inclined to speculate.

Yet the old experiment of Schwann † has always been a stumbling-block in the way of any theory that would explain muscular contraction by the action of a force which must increase inversely as the square of the distance between the molecules, for the force of the contraction, as it actually occurs, diminishes as the muscle shortens; and hence we find so good a physiologist as Radcliffe ‡ reviving in a modified form the old hypothesis of Matteucci §, in accordance with which the electrical tension of the fibre in the state of rest causes a mutual repulsion of the molecules, and so elongates the muscle, while the contraction is merely the effect of the elasticity of the tissue, which asserts itself so soon as the repulsive force is diminished by the negative variation that precedes contraction.

In consequence of these and other difficulties many physiologists are beginning to regard the electrical phenomena as subordinate accidents of the chemical processes that go on in muscle, and endeavour to explain muscular contraction as resulting directly from these chemical processes themselves. Arthur Gamgee ‖ has adopted as most probable the chemical hypothesis of Hermann ¶. This assumes the contraction to result from the decomposition of a complex nitrogenous compound supposed to be contained in the muscular tissue, and

* Mayer, Müller's 'Archiv,' 1854, S. 214; Amici (1858), translation in Virchow's 'Archiv,' Bd. xvi. 1859, S. 414.

† Schwann, in Müller's Handb. der Phys. 1837, Bd. ii. S. 59.

‡ C. B. Radcliffe, 'Dynamics of Nerve and Muscle' (London, 1871).

§ Matteucci, 'Lectures on the Physical Phenomena of Living Beings' (translated by J. Pereira), London, 1847, p. 333.

‖ Arthur Gamgee, 'A Text-Book of the Physical Chemistry of the Animal Body,' vol. i. (London, 1881), p. 418.

¶ L. Hermann, 'Grundriss der Phys. des Menschen,' 5te Aufl. 1874, S. 231.

named inogen. During contraction inogen breaks down into carbon dioxide, lactic acid (Fleischmilchsäure), and gelatinous myosin. The rearrangement of molecules necessary to produce the latter body determines the contraction. Subsequently the gelatinous myosin combines with the necessary materials furnished by the blood, and becomes inogen again. This decomposition and recomposition goes on also while the muscle is at rest; but as then the gelatinous myosin is reconverted into inogen as rapidly as it is formed, no contraction results.

Du Bois-Reymond declares all this to be merely unsupported hypothesis *. Gamgee himself admits that it is after all not very clear why the gelatinous myosin should contract. Michael Foster †, who wholly rejects this particular chemical hypothesis, nevertheless seems quite sure that the true explanation will be found to be a chemical one. He insists that muscular contraction is essentially a translocation of molecules, and declares that whatever the exact way in which this translocation is effected may be, it is fundamentally the result of a chemical change, or, as he describes it, "an explosive decomposition of certain parts of the muscle-substance."

The purpose I have in view does not require, fortunately, that I should attempt to decide whether these more purely chemical theories of muscular contraction or the more purely electrical theories are best entitled to confidence. My object has been effected if I have impressed you with the fact that wide differences of opinion still exist as to the nature of the process, and that further investigation is indispensable for the settlement of existing controversies.

The subject thus briefly discussed brings us naturally to the consideration of the nature of the action of the motor nerves, by which, in all animals possessed of a muscular and nervous system, the contraction of the muscles is regulated and determined.

The hypothesis which identifies the nervous currents with electricity was propounded in the posthumous work of Hausen‡ in 1743, and, notwithstanding all the difficulties and objections it has encountered, still survives in a modified form in many contemporaneous minds. Those who hold to this view appeal in its support to the electrical phenomena actually observed in nerves in accordance with the investigations of

* Du Bois-Reymond, Ges. Abh. Bd. ii. S. 320.

† Foster, *op. cit.* p. 79 *et seq.*

‡ C. A. Hausen, 'Novi profectus in historia electricitatis' (Leipsic, 1743). I cite from Du Bois-Reymond, 'Unters. über thierische Elektricität,' Bd. ii. (Berlin, 1849), Th. i. S. 211.

Du Bois-Reymond. These observations have long been widely accepted as conclusive proof that natural currents exist in the quiescent nerve of the same general character as those attributed to the quiescent muscle, which I outlined a few minutes ago. The electromotive force of this current was found by Du Bois-Reymond * to be equal to ·022 Daniell in the sciatic nerve of the frog. When a nervous impulse passes along the nerve, the natural current is diminished; it experiences a negative variation, which, according to Bernstein †, when the impulse results from a very potent stimulation, may more than neutralize the natural current. The same physiologist has shown that this negative variation moves along the nerves of the frog at the rate of 28 metres per second, that is, at the same rate as the nervous impulse itself, as determined without reference to the electrical phenomena.

As in the case of the muscle-currents, these phenomena have been differently interpreted by Hermann ‡, who denies the existence of any natural nerve-current in uninjured nerves, and ascribes those observed in the experiments to the circumstance that the parts of the nerve dead or dying, in consequence of the section, become negative to the living nerve. The negative variation produced by the stimulation of a nerve he explains by assuming that the stimulated part of the nerve becomes, in consequence of the changes resulting from the stimulation, negative to the unstimulated parts. I will not attempt to enter to-night into the merits of the controversy still in progress with regard to this question, nor will I pause to discuss the exceedingly curious and interesting phenomena of electrotonus §, concerning which I will only say that the question has even been raised by Radcliffe as to how far these phenomena are peculiar to nerves, and how far they may be regarded as mere phenomena of the electrical currents employed, which would be equally manifested under similar circumstances if a wet string or other bad conductor should be substituted for the nerve ‖.

However these disputes may be ultimately decided, whatever the actual facts with regard to the electrical manifestations in nerves at rest or in action may ultimately prove to

* Du Bois-Reymond, Ges. Abh. Bd. ii. S. 250.

† Bernstein, *op. cit. suprà.*

‡ Hermann, *loc. cit. suprà*, note *, p. 242; also Handb. der Phys. Bd. ii. Th. 1 (Leipsic, 1879), S. 144 *et seq.*

§ See especially Du Bois-Reymond, 'Unters. Bd. ii. Th. 1, S. 289, and Pflüger, 'Unters. über die Physiologie des Electrotonus' (Berlin, 1859). An excellent summary of the observations (with the literature) is given by Hermann, 'Handb. der Physiologie,' Bd. ii. Th. 1, S. 157 *et seq.*

‖ Radcliffe, p. 74 *et seq.*, *op. cit. suprà.*

be, there is a group of easily repeated elementary experiments which seem to show pretty distinctly that whatever the nervous impulse may be it is not merely an electrical current.

It was known already when Haller wrote * that a string tied tightly around a nerve, although it in no wise interferes with the passage of electrical currents, puts a speedy end to the transmission of nervous impulses. With this old experimental difficulty uncontradicted, it seems strange that any one should declare at the present time that "the main objection raised to the electrical character of nerve energy is based upon its slow propagation" †. In fact this latter objection is altogether a subordinate difficulty which may perhaps be entirely explained away; the main experimental objection does not relate to the velocity, but to the conditions of the propagation of the nervous impulse. If instead of tying a string around it the nerve be merely pinched or bruised well with a pair of forceps, so as to destroy its delicate organic texture, if it be compressed tightly by a tiny metallic clamp, if it be divided by a sharp knife, and the cut ends brought nicely into contact, or brought into contact with the extremities of a piece of copper wire, it will still conduct electrical currents as well as ever, but can no longer transmit the nervous impulse. So, too, there are certain poisons, such as the woorara, which completely destroy the capacity of the nerve for transmitting nervous impulses without in the least diminishing its conductivity for electricity ‡.

In view of these and other practical difficulties, the best instructed modern physiologists no longer attempt to identify the nervous impulse with the electrical phenomena by which it is accompanied. Du Bois-Reymond himself has suggested that the nervous agent "in all probability is some internal motion, perhaps even some chemical change, of the substance itself contained in the nerve-tubes, spreading along the tubes" §. Herbert Spencer came to the conclusion that "nervous stimulations and discharges consist of waves of molecular change" || flowing

* A. von Haller, 'Elementa Physiologiæ,' lib. x. sect. viii. § 15, t. iv. (Lausanne, 1762), p. 380. He cites as authority the essay of Le Cat, crowned by the Berlin Academy in 1753. [We have in the S. G. O. Library the Berlin edition of 1765, 'Traité de l'existence &c. du fluide des nerfs,' &c.]

† Barker, p. 8, *op. cit. suprà.*

‡ Claude Bernard, 'Leçons sur la Phys. et la Path. du Système nerveux' (Paris, 1858), t. i. pp. 157 and 224.

§ Translation of a lecture given by E. Du Bois-Reymond at the Royal Institution, London, in Appendix no. 1 of H. Bence Jones's 'Croonian Lectures on Matter and Force' (London, 1868), p. 130.

|| Herbert Spencer, 'The Principles of Psychology,' vol. i. (New York, 1871), p. 95. Compare also his 'Principles of Biology,' vol. ii. (New York, 1867), p. 346 *et seq.*

through the nerve-fibres; and I suppose that most physiologists at the present time think of the nervous current in some such way as this. Even those who attach most importance to the electrical phenomena will, I take it, agree with Michael Foster that these "are in reality tokens of molecular changes in the tissue much more complex than those necessary for the propagation of a mere electrical current" *.

We do not, however, as yet possess any sufficient foundation of facts on which to build a reasonable hypothesis as to the nature of the molecular disturbances that accompany a nervous impulse. The labours of the physiological chemists have taught us nothing with regard to the changes that go on, except that the axis-cylinder, which in the inactive living nerve is alkaline, becomes acid after long-continued activity or after death †. We can measure the velocity with which the impulse travels, we can study the conditions under which it arises, we can believe, as I certainly do, that it will ultimately receive a chemico-physical explanation; but its real nature we do not yet know.

So far as we can ascertain, the phenomena of the conduction of nervous impulses by the sensitive nerves are so similar to those of the conduction of motor impulses that any explanation ultimately adopted for the one will probably apply to the other also. When, however, we ascend to the study of the nervous centres, by which sensitive and motor nerves are connected together, and attempt the interpretation of the complex functions of nerve-cell, ganglion, spinal cord, and brain, we find that none of the hypotheses hitherto brought forward to explain the observed phenomena repose on any defensible chemico-physical basis.

I cannot of course undertake to give to-night even the most meagre outline of the wondrous mechanism which physiological experiments show must exist. That reflex actions, co-ordinated muscular movements, and all the complex phenomena of this class do depend upon a wonderfully complex mechanism, and occur in strict accordance with the ordinary chemical and physical laws, I do not for a moment doubt, and I cordially invite the cooperation of the chemists and physicists to aid the physiologists in the explanation of this mechanism, for we stand only upon the threshold as yet.

If now we turn from the more general discussion of muscular contraction and nervous action to the consideration of the several functions carried on in animals by means of special arrangements of the muscular and nervous systems,

* Foster, p. 79, *op. cit. suprà.*

† A Gamgee, p. 447, *op. cit. suprà.*

we continually encounter the preponderating influence of purely physical laws. The introduction of air into the lungs of breathing animals and its expulsion thence is effected in a purely mechanical way, while the exchange of the carbon dioxide of the blood with the oxygen of the inspired air occurs in strict obedience to the laws of the diffusion of gases.

The ordinary laws of hydraulics govern the circulation of the blood and lymph, and all the complex visible motions of the body are executed in accordance with the ordinary laws of mechanics; nor is it at all necessary for me to insist upon the purely physical nature of the operations of the organs of the special senses, conspicuously the eye and the ear. For example, so far as concerns the means by which images of external objects are formed sharply upon the retina, the eye is as purely a physical instrument as the telescope or the microscope. But I need not dwell upon this group of phenomena, because the importance of the rôle of the ordinary physical laws in this domain is conceded, I suppose, by the extremest of the vitalists of the present day.

We see, therefore, that, with regard to a large part of the phenomena of living beings, there are grounds for affirming either that they have already been satisfactorily explained by a reference to established chemical and physical laws, or at least that they are of such a character that it is reasonable to hope they may be thus explained at some future time. Is it possible, then, to return, as some have done of late years, to the old speculation of Des Cartes, and look upon living beings as mere machines? To do so it will not suffice to image to yourselves ordinary machines in which fuel yields force. To satisfy the chemico-physical hypothesis of life you must suppose machines that build themselves, repair themselves, and direct from time to time new applications of their energy in accordance with changes in the environment—nay, more, machines that accouple themselves together, breeding little machines of the same kind, that grow by and by to resemble their parents, and all this self-directed, without any engineer. But even Des Cartes required an engineer—the soul—to run his man-machine; and the logic which compelled him to this view applies just as forcibly to all the modern machine-conceptions of living beings.

I have already asserted that there are whole groups of phenomena characteristic of living beings, and peculiar to them, which cannot be intelligently explained as the mere resultants of the operation of the chemical and physical forces of the universe. These phenomena I refer—I avow it without hesitation—to the operations of a vital principle, in the existence of which I believe as firmly as I believe in the existence of force, although I do not know its nature any more than I know the nature of force. If, for convenience, at any time I compare the living body to a machine, I must compare the vital principle to the engineer; it is the director, the manager if you will, but it does not supply the force that does any part of the work. Let us consider, then, in the remainder of this discourse the phenomena which indicate the guidance of the vital principle.

The first group of phenomena belonging to this second class are those forced upon our attention whenever we attempt to study the question of the origin of life. It has seemed to some of our contemporaries that, in accordance with the doctrine of evolution, as deduced by Mr. Herbert Spencer from the great truth of the persistence of force, life ought always to arise spontaneously out of inorganic matter whenever the necessary materials and other conditions of life are brought together. Indeed, if there be nothing more or other in life than force, I confess I do not understand how this conclusion can be logically escaped; and yet when we come to interrogate nature we find that, in point of fact, things do not happen so.

The sun may stream all the enormous energy of his rays upon the slime of the Nile, but he generates no monsters; nay, not even a bacterium, except in the presence and under the direction of pre-existing life. Our biological knowledge has so far advanced that it is easy for us to get together mixtures of matter, for the most part derived from pre-existing living beings, which are peculiarly well fitted to supply the materials needed for the building up of a variety of low forms of life; and the extent of our present knowledge of the conditions favourable to the development of these low forms of life is shown by the rapidity with which they do develop from a few individuals to countless millions, if only a few individuals are introduced as parents into our flasks and brood-ovens. The species to which the countless progeny belongs, depends always upon the species of the parents we introduced by design or accident; and if parents of several species are introduced we may imitate on a tiny scale the great struggle for existence, and witness the survival of the fittest. Never, however, has the spontaneous generation, out of inorganic matter, of a single living form been yet observed.

Speculative considerations have, indeed, from time to time led certain enthusiasts to desire earnestly that it might be observed; and when we consider, on the one hand, the influence of pre-existing bias, and, on the other, the intricacy of some of the experimental processes in question, it is by no means necessary to charge dishonesty upon those who, from time to time, have actually fancied that their desires have been realized to the extent of the spontaneous generation of bacteria at least. When we consider the immense development of the trade in canned food, which could not exist for a single summer's day if these experimenters were not mistaken, it will be seen how little need there was for renewed scientific experiment to refute their conclusions; but it is a noteworthy fact that among those who have contributed most by exact research to recent scientific demonstrations of the truth that life never arises except from pre-existing life, are to be found some of the most earnest and eloquent advocates not merely of the doctrines of evolution, but of its supposed corollary, the chemico-physical hypothesis of life.

I sympathize heartily with those who, recognizing that the supposition of the spontaneous origin of life on our globe is flatly contradicted by the facts of science, have endeavoured to escape the difficulty by imagining the earliest parent living forms to have been brought to our earth on the surface of meteoric stones or other cosmical bodies. This hypothesis, put forward originally on purely theoretical grounds, has recently acquired a certain degree of support from the published observations of Hahn and Weinland*, who believe they have recognized the remains of humble coralline forms in thin sections of meteoric stones collected in Hungary. Yet these observations, if indeed they should prove to be correct, would rather afford indications of the existence of life in other worlds than ours, than show that living forms could survive the high temperature to which such cosmical masses must be exposed during their transit through our atmosphere; and even should we find reasons for ultimately adopting this hypothesis, we should not have solved the problem of the origin of life, but only removed it entirely beyond the domain of further scientific investigation.

If, however, we reject this view, and still mean to support the chemico-physical hypothesis of life, we shall have to resort to a still more improbable supposition. We shall have to suppose that although in the present order of things life can only arise out of pre-existing life, the order of things was at some past time so far different that life could then arise out of inorganic matter—a supposition which implies an instability in the course of nature that is contradicted by all the teachings of science.

* O. Hahn, 'Die Meteorite und ihre Organismen,' Tübingen, 1881. I cite the Journ. of the Royal Microsc. Soc. October 1881, p. 723.

I willingly admit that, in view of our present scientific notions of the cosmogony, it is impossible to believe that life always existed upon this planet. I willingly admit that life on the earth must have had a beginning in time. But we do not know how it began. Let us honestly confess our ignorance. I declare to you I think the old Hebrew belief, that life began by a creative act of the Universal Mind, has quite as good claims to be regarded as a scientific hypothesis as the speculation that inorganic matter ever became living by virtue of its own forces merely.

If we turn now to the consideration of the processes of growth, we shall find additional reasons for believing in the existence of a vital principle. Let us consider first, in the most general way, the conditions under which those strictly chemical processes occur, to which I have already alluded, and by which the inorganic atoms are combined into organic matter. I repeat it, I do not for a moment question that the actual force by which these processes are compelled exists in the solar rays, and that it is, after all, the solar energy thus stored up in the vegetable protoplasm and its products that supplies, by its subsequent liberation, all the force manifested by living beings. Yet, let me beg you to observe that in all the myriads of years during which the solar energy has streamed upon the earth, that energy has never, on any occasion that we know of, determined the combination of inorganic atoms into organic matter, except within the substance of already living protoplasm. The water and carbon dioxide and ammonia in the atmosphere and in the soil come into contact with each other, within the substance of porous inorganic clods on the surface of the soil, much as they do in the substance of protoplasm, and the equal sun warms both alike; but in the clod they remain water, carbon dioxide, and ammonia; in the protoplasm, provided only that it is living protoplasm, they combine into starch or oil, or even into protoplasm itself. The essential condition, then, of this storing up of the solar energy for the subsequent use of living beings is the presence of life, and in these fundamental operations the mighty force of the sun acts, in the fullest sense of the words, the part of the servant of life.

The view thus suggested, that we have here to do with something more than the mere operation of the inorganic forces, is still further strengthened when we come to consider more in detail the phenomena of the growth of living beings, whether plants or animals. The better we become acquainted with these phenomena the more fully we become convinced that we have to do with processes for which the inorganic world affords no parallel.

Linnæus, indeed, declared, "lapides crescunt," using the very same phrase which he applied also to plants and animals*. But it is impossible to maintain this assertion without adopting the most superficial view of the growth of living beings, and defining the process to consist merely in increase of size. That this should have appeared reasonable in the time of Linnæus need excite no surprise; but it seems strange to find so astute a thinker as Mr. Herbert Spencer repeating the old fallacy in the first chapter of his 'Inductions of Biology,' and declaring, "Crystals grow, and often far more rapidly than living bodies"†. Then, after instancing the formation of geological strata by the deposit of detritus from water, as well as the formation of crystals in solutions, as examples of growth in the inorganic world, he asks: "Is not the growth of an organism a substantially similar process?" and adds, "Around a plant there exist certain elements that are like the elements which form its substance, and its increase in size is effected by continually integrating these surrounding like elements with itself; nor does the animal fundamentally differ in this respect from the plant or the crystal."

Now, as opposed to this, I must express my belief that the more we know of the actual details of the process of growth in plants and animals the more clearly will it be seen that this process does differ so fundamentally from that by which a crystal is formed and increases in size, or from any increase in size of inorganic bodies, that the same scientific term cannot with any propriety be applied to both, however long popular usage may have given to both a common name. When inorganic bodies increase in size the additional atoms are deposited on their external surfaces; or, if a fluid, after penetrating the interstices of some porous body, deposits there any material held in solution, the mass, indeed, is increased thereby, but not the size. When, however, vegetable protoplasm grows, it does not merely integrate with itself certain elements around it like the elements which form its substance; the needed elements exist in compounds quite unlike itself, and it combines them together into protoplasm in all parts

* "Lapides crescunt, Vegetabilia crescunt et vivunt, Animalia crescunt, vivunt et sentiunt." This phrase occurs in the first edition of the 'Systema Naturæ,' Leyden, 1735. I cite the reprint of Fée, Paris, 1830, p. 3, as well as the second Stockholm edition, 1740, p. 76. The expression is replaced in the later editions by more guarded language.

† Herbert Spencer, 'The Principles of Biology,' vol. i. New York, 1866, p. 107.

of its mass, so that it grows by a process of intussusception wholly unlike anything that occurs in the inorganic world. In the case of animal protoplasm, the mode of growth by intussusception is the same, but the capability of combining together mere inorganic elements into its own substance is lost; and, besides these, a certain amount of pre-existing vegetable or animal protoplasm must be present in the food, or growth will not go on.

In both cases, when the growth has proceeded to a certain extent—within certain definite limits—a new characteristic phenomenon occurs in a growing mass of vegetable or animal protoplasm; it multiplies by division, its whole mass participating in the act, in accordance with one or other of a few definite methods. This process is repeated again and again. The progeny may separate, without modification, as independent forms, or, as in the case of the more complex organisms, they may cohere together, and the process culminates by groups of them undergoing certain definite and peculiar transformations, after which further multiplication becomes rare or ceases altogether, and the growth of the complex organism is thus limited.

I cannot, of course, attempt this evening to describe all the known details of the progress of growth which I have thus hastily sketched; to give you a really satisfactory account of them would require a series of lectures. But I do not hesitate to say that the more fully you know these details the more unscientific you will think the attempt to class them as in any way similar to the circumstance that inorganic crystalline compounds seem "each to have a size that is not usually exceeded without a tendency arising to form new crystals, rather than to increase the old." It is, at the best, a waste of words to attempt to explain complex phenomena by comparing them to simpler ones which are fundamentally unlike them.

I have but now referred to a process by which, in the growth of the more complex living beings, the small primitive protoplasmic mass, out of which each individual arises, subdivides and produces a numerous brood of protoplasmic masses, at first closely resembling the parent mass, but after a time differing from it more and more, and finally undergoing transformations into definite and peculiar forms. This process, which does not take place in any disorderly manner, but in a very characteristic and definite way in each individual form, is designated by the term development. In point of fact, so far as it consists in the mere growth and multiplication of the individual elements that compose the organism, and the increase in size of the organism itself on account of these processes, it is properly designated by the term growth. In so far, however, as the individual elements are differentiated, and the wonderful architecture of the living being, with its organs and systems, is completed thereby, it is properly designated by the term development.

Nothing like the process of development as thus defined exists in the inorganic world, and in all the attempts at such a comparison that it has been my fortune to meet with, the most fundamental facts of the development of living beings have been persistently ignored. Among these fundamental facts I invite your attention especially to the circumstance that there is something in the microscopic mass of protoplasm, out of which, even in the case of the highest and most complex living beings, each individual arises, that goes even further in determining the direction in which the individual shall develop than the pabulum, or environment, or all the mighty chemical and physical forces that are brought into play as the process goes on. In a word, the individual develops after the pattern of its parent, or not even all the solar energy can compel it to develop it at all.

We are thus brought face to face with the facts of sexual generation, and especially of heredity, with all their wide bearings on the great biological questions of natural selection and the origin of species. Into the details of these large questions the limits of the hour will not permit me to enter. Could I take time to do so, I am satisfied that at every step I should be able to collect for you additional evidence of the existence of a vital principle. Still, I regret this the less because most of you, I think, are so familiar with the modern literature of these subjects, and especially with the admirable writings of Mr. Darwin, that I feel sure, if I can succeed in giving you a clear outline of my views, much that I should say, had I time, will suggest itself to your own minds. In a general way, however, when we study, in the history of life upon this globe, the double phenomena of long-continued persistence of type, and of slow variation continually occurring, we shall find that almost all biologists, whatever their theory of life, explain these phenomena on the one hand by heredity, on the other by the sensibility of the organism to the influence of the environment.

Both heredity and the influence of the environment may be very conveniently studied in those simplest organisms in which each individual consists of a single minute mass of naked protoplasm, as in certain rhizopods, for example, the Amœba. These tiny creatures produce a progeny which preserves the parental type as closely as is done by the offspring of the higher animals. Their sensibility to the influence of the environment is manifested in several ways. They grow, that is they appropriate materials from the environment, in the way I have already specified; they manifest automatic movements, that is, on encountering food, obstacles, or other disturbing external circumstances, movements result the direction and energy of which are in no wise determined by the character or force of the external influences, or, as they may be conveniently termed, the stimuli by which these movements are provoked; and finally, simultaneously with the process of growth, a certain metamorphosis, or metabolism, of the protoplasm is continually going on, resulting in the formation of excrementitious substances which are continually being excreted.

The processes of growth and metabolism exhibit different degrees of intensity in accordance with variations of the environment; and whatever physical theory of the mode in which the protoplasmic motions are produced we may adopt, the mechanical force manifested can only be supposed to proceed from the decomposition of a part of the protoplasm itself into simpler compounds, that is, from a particular kind of metabolism. Hence you will, I think, be quite prepared to hear me speak of all the circumstances in the environment that so act upon living protoplasm as to increase its growth or metabolism as stimuli, and of the property of living protoplasm by which all its responses to stimuli are guided as irritability, instead of limiting these terms to the phenomena of automatic movement only, as was formerly done. This irritability of living protoplasm determines the direction in which its internal forces shall be manifested. Speaking of it as I do, perhaps you would wish me to call it sensibility rather than irritability; and I do not know that I should object very strenuously to any one who wished to do this. But however you may name it, it is this vital property of all living protoplasm that produces the sensibility to changes in the environment, which has been the main factor in the gradual evolution, during the ages, of the highest and most complex from the simplest and lowest living forms.

Against this view it has been urged with much ingenuity that protoplasm is the material substratum of life, and life merely a property of protoplasm; that is, if the words have any meaning at all, that life is the resultant only of the forces inherent in the inorganic atoms of which the protoplasm is built up. Now, in the first place, no one has ever yet been able to show, by any conceivable synthesis, how the forces known to belong to the several kinds of inorganic atoms, of which protoplasm is composed, could by their combination, produce the characteristic phenomena of living protoplasm, namely, the phenomena of irritability, as I have just described them. But, in the second place, this speculation appears to be pretty flatly contradicted by the circumstance that, although protoplasm can only be formed within the substance of previously existing living protoplasm, it can continue to exist, it does continue to exist as protoplasm after it has ceased to live. Not merely can it persist for a time without chemical change as dead protoplasm, it can subsequently serve as food and be reconverted into living protoplasm once more. Bear in mind, however, that this change can only be effected within the substance of the living protoplasm of the animal that assimilates this food. It is not effected by the chemistry of digestion; that merely makes peptone of the protoplasm—merely makes it soluble enough to pass into the substance of the protoplasmic masses that are to appropriate it. These considerations, then, would seem to show that the material, protoplasm, cannot be rightly believed to be of itself the cause and essence of life.

If I should pause here, it seems to me that I should have brought forward adequate reasons for believing in the existence of a vital principle. But I cannot pause here. Beyond and above all this there is another great group of phenomena peculiar to living beings—a group of phenomena concerning which, in my own individuality, I have knowledge at least as positive as any I possess of the existence of force, and which I am led, by a logic quite as convincing as that by which any general proposition with regard to the external world is proven, to believe exists in like kind and degree in the case of my fellow-man. I refer to the phenomena of the perceiving, emotional, will-full, reasoning human mind. Into the argument that makes it highly probable that a similar but less and less perfect mind exists in the animal world, and identifies with mind the sensibility of the lowest animal forms, and even that of vegetable protoplasm, I will not attempt to enter to-night. Mr. Herbert Spencer himself has presented this view with so much ingenuity, that, without committing myself to an approval of all his details, I must content myself by referring you to his writings for one of the best discussions of this matter. It will be sufficient for my present purpose to close this discourse by the presentation of a few considerations in relation to mind as it exists in man.

For myself I know mind only as a manifestation of life, if, indeed, it is not the essence of life. But the old doctrine of Epicurus, handed down to us in the poem of Lucretius, that in some way or fashion mind is produced by the clashing together of the atoms, has been boldly revived of late years, and transmuted into a form more plausible to modern thought, although just as unsupported by any actual knowledge of facts.

No one has done this more boldly or more cleverly than Mr. Herbert Spencer has done in his 'First Principles,' and of course you are all familiar with the ingenious argument, in favour of this view, which runs through that masterly work. It would be, from many points of view, profitable, but it would be a very laborious task, to attempt the critical discussion of his argument. It must suffice, for my present purpose, to point out that two of the fundamental assumptions upon which that argument is based are wholly undemonstrated. The first assumption is, that mind is itself a force*; the second, that mind cannot be conscious of itself, but only of the external world†.

If I could bring myself to believe that mind is, in any proper sense of the word, a force, and that such popular metaphorical expressions as mental force or mental energy accurately described the phenomena, I should certainly expect to find at least some shadow of proof for Mr. Herbert Spencer's assertion that mental operations fall within the great generalization of the correlation and equivalence of the forces. On the contrary, however, you will find, on reading his lucid periods, that his whole argument relates to those physical conditions in the organs of sense and in the muscular and nervous systems which are the antecedents of perception—which are, in fact, the things really perceived—and in no sense constitute the perceiving mind. Between strictly mental phenomena and the physical forces no one has as yet even attempted to establish a numerical equivalent; nay, more, the correlation of thought with the physical forces is not only undemonstrated, it is utterly unthinkable. You can conceive several different ways, it matters not whether true or false, in which the motions we know as heat might be converted into those we know as light, and so on with the other physical forces; but you cannot represent mentally any intelligible scheme by which any of the physical forces can be converted into the simplest or most elementary thought.

As to the question of self-consciousness, it seems as if the great philosopher were reasoning in a circle. He first assumes that the fundamental condition of all consciousness is the antithesis between subject and object,—which is true only

* Herbert Spencer, 'First Principles,' Amer. Ed. New York, 1864, p. 274.

† Id. *op. cit.* p. 65 *et seq.*

with regard to consciousness of perception, the form of consciousness by which we become acquainted with the non ego,—and then he concludes that there can be no consciousness of the ego because it cannot fulfil these conditions. That is, in a word, he denies consciousness of the ego, because it is not consciousness of the non ego. Really it appears to me that, as against such a philosophy as this it is not amiss to appeal to "the unsophisticated sense of mankind," of which Mr. Mansel speaks*. But there is fortunately a better philosophy than this—a philosophy which recognizes the validity of the mind's self-consciousness as at least fully equal to the validity of its consciousness of the conditions of the body by which it obtains a knowledge of the external world. By this self-consciousness I know, with a certainty which no doubt can ever disturb, that I have a mind; and by rightly applying my reasoning powers to the data of my self-consciousness, I can learn much that will be useful to me with regard to my mental processes and the methods of employing them. But here I have to stop. I can learn nothing, whether by consciousness or by reasoning, with regard to the real nature of my conscious mind, and however much it may long for immortality, neither philosophy nor science affords any foundation of proof upon which it might build its hopes.

I have already said that I know mind only as a manifestation of life. Its operations are intimately connected with the chemical and physical phenomena of living beings, and it exercises over them a certain directing influence, the nature of which we do not understand. The obedience of our voluntary muscular actions to the mandates of the guiding will is a familiar illustration of this directing influence. On the other hand, all the knowledge of the external world on which the mind exerts its reasoning power reaches it through the organs of sense and the nervous system. Indeed, our studies of the phenomena of sensation compel us to conclude that what our mind really perceives, when it takes cognizance of the external world, is merely the ever-changing panorama of our own cerebral states. It should be anticipated therefore that disturbed or morbid conditions of the brain would lead to irregular or disorderly mental operations; and the circumstance that this really happens affords no better proof of the materiality of thought than is afforded by the circumstances of our ordinary normal thought.

So, too, since the cerebral changes, which the mind perceives, are themselves of a purely chemico-physical nature, it should be anticipated that, like the metabolic processes in

* As cited by Mr. Herbert Spencer, *loc. cit.* last note.

other tissues, they would be accompanied by an increased excretion of characteristic waste-products, by evolution of heat and by afflux of blood. Experimental investigation has been directed to each of these points, and some important observations have no doubt been made; but much of the testimony is conflicting, and our knowledge is still so incomplete that further inquiry in each direction is greatly to be desired.

This is particularly the case with regard to the chemical questions connected with the metabolism of the brain. In the first place, our knowledge of the chemical composition of brain-substance is still in its infancy. The view that its characteristic ingredient is the phosphorized nitrogenous body described in 1865 by Liebreich under the name of protagon has been strongly controverted by Diaconow, Hoppe-Seyler, and Thudicum, while recently it has been reaffirmed by Gamgee and Blakenhorn*. But even should this view turn out to be well founded, we have yet every thing to learn with regard to the transformations protagon undergoes during functional activity, and the nature of the resulting waste products.

Long before Liebreich announced the existence of protagon, however, the attention of the physiological chemists had been directed to the prominence of phosphorus as an element in the composition of the cerebral substance, and it had been suggested that a part of the phosphoric acid excreted in the urine might be derived from the metabolism of the brain. As early as 1846 Bence Jones † had observed an excess of phosphatic salts in the urine during certain brain-diseases, notably acute inflammations; and an observation published in 1853 by Mosler ‡ appeared to indicate that a similar excess followed intellectual activity.

Byasson [1868] in his essay on the relation between cerebral activity and the composition of the urine§, reports a number of urinary analyses which support the view that the excretion of alkaline phosphates by the kidneys is habitually increased during mental work. This opinion has also received a certain degree of support from the more recent papers of Zülzer ‖ and Strübling ¶; nevertheless it is impossible to

* Gamgee, p. 425 *et seq. op. cit. suprà.*

† Henry Bence Jones, "On the Variations in the Alkaline and Earthy Phosphates in Disease," Phil. Trans. for 1846, p. 449.

‡ Mosler, "Beiträge zur Kenntniss der Urinabsonderung," &c., Inaug. Diss., cited in Canstatt's Jahresbericht, 1853, Bd. i. S. 134.

§ H. Byasson, "Essai sur la relation qui existe à l'état physiologique entre l'activité cérébrale et la composition des urines," Paris, 1868.

‖ W. Zülzer, "Ueber das Verhältniss der Phosphorsäure zum Stickstoff im Urin," Virchow's Archiv, Bd. lxvi. 1876, S. 223.

¶ Strübling, "Ueber die Phosphorsäure im Urin," Archiv für exp. Path. und Pharm., Bd. vi. 1876–77, S. 266.

study the detailed observations upon which it is based without feeling how meagre and unsatisfactory the evidence relied upon really is. It is, at best, only sufficient to indicate the importance of further inquiry, and to suggest the necessity of avoiding certain obvious errors of method which complicate and obscure the results of the investigations hitherto made.

The opinion that mental effort is accompanied by an increase in the temperature of the brain was first propounded by Lombard in 1867. Using a delicate thermo-electric apparatus of his own contrivance, he observed during mental effort a rise of the surface temperature of the head, which sometimes amounted to as much as one-twentieth of a degree centigrade*. Subsequent and more elaborate investigations confirmed him in this conclusion, which has also been supported by observations made with thermo-piles by Schiff and Bert, as well as by the use of surface thermometers in the hands of Broca and L. C. Gray of Brooklyn†. Gray claimed to have observed a maximum rise of as much as two and a half degrees Fahrenheit. These physicians and some others have also investigated the relative temperature of the two sides of the head, of different regions on each side, the variations produced in certain regions by voluntary muscular movements, and those resulting from localized brain-diseases‡.

To attempt any discussion of these interesting studies and their conflicting results would lead me altogether beyond my prescribed limits. It is enough for my present purpose to

* J. S. Lombard, "Experiments on the Relation of Heat to Mental Work," The New York Medical Journal, vol. v. 1867, p. 199.

† J. S. Lombard, "Experimental Researches on the Temperature of the Head," Proc. of the Royal Society of London, vol. xxvii. 1878, p. 166; Idem, "The Regional Temperature of the Head," London, 1879; Idem, "Experimental Researches on the Temperature of the Head," London, 1881. Moritz Schiff, "Recherches sur l'échauffement des nerfs et les centres nerveux à la suite des irritations sensorielles et sensibles," Achives de Physiol. norm. et path. t. iii. 1870, p. 5 *et seq.* Bert, Communication to the Société de Biologie, read Jan. 18, 1879, in 'Gazette Hebdomadaire,' Jan. 24, 1879, p. 63. Broca, Communication to the French Association for the Advancement of the Sciences, at the Havre meeting of 1877, in Gaz. Hebd., Sept. 7, 1877, p. 577; also Gaz. Méd. de Paris, 1877, p. 457; Idem, in London Med. Record, Jan. 15, 1880. L. C. Gray, "Cerebral Thermometry," The New York Med. Journ. vol. xxviii. 1878, p. 31; also 'Chicago Journ. of Nervous and Mental Diseases,' vol. vi. 1879, p. 65.

‡ See, besides the papers cited in the last note, C. K. Mills, in the New York Med. Record, vol. xiv. 1878, p. 477, and vol. xvi. 1879, p. 130; Maragliano and Seppelli, "Studies on Cerebral Thermometry in the Insane," translated by J. Workman, 'The Alienist and Neurologist,' St. Louis, Jan. 1880, p. 44 *et seq.*; R. W. Amidon, "The Effect of willed Muscular Movements on the Temperature of the Head," 'Archives of Medicine,' April 1880, p. 117.

point out that the recent investigations of François Frank * would seem to indicate that the variations of temperature actually observed are chiefly due to changes in the cerebral circulation. Plunging suitable sounds, connected with a thermo-electric apparatus, into the brains of animals to different depths, Frank found that the deeper parts of the brain are always warmer than its superficial layers. The superficial layers are continually cooled by radiation, and their temperature is a degree, or more than a degree, centigrade lower than that of the deeper parts. Even these, however, are from 1° to 2° centigrade cooler than the blood in the thoracic aorta; and it will therefore readily be understood that a relaxation in the muscular coats of the cerebral vessels, permitting the more rapid circulation of a larger quantity of blood, would be promptly followed by an increase in the temperature of the superficial parts of the brain. None of the observers I have cited have reported a surface temperature of the head during mental effort that is too high to be accounted for in this way; and if, as I willingly concede is probable, there is really an increased heat-production in the brain itself, it is wholly masked by the more considerable change due to afflux of blood.

Now a consideration of the phenomena of blushing, and certain well-known sensations in the head, might lead us to expect that emotional and mental conditions would prove to be attended by increased activity in the circulation of the blood in the brain; yet many difficulties have hitherto been encountered in the attempt to demonstrate experimentally that this is true. Mosso of Turin supposed that he had succeeded in doing this with his plethysmograph †. The instrument is essentially a cylinder of water, into which the arm is introduced and so fastened in place by a caoutchouc membrane that the slightest increase or diminution in the volume of the arm will cause the rise or fall of the water, through a tube connected at one end with the interior of the cylinder and at the other with a suitable recording apparatus. The pen or pencil of this apparatus inscribes a curve that rises or falls with the fluid in the tube. Among the curious observations made with this instrument, Mosso reports that the mental operations and emotions of the persons he experimented on

* François Frank, Communication to the Société de Biologie, May 29, 1880, in Gaz. Hebd., June 11, 1880, p. 392.

† Angelo Mosso, "Sopra un nuovo metodo per scrivere i movimenti dei vasi sanguini nell' uomo," Atti della Reale Accademia delle Scienze di Torino, t. xi., Nov. 14, 1875. I have not obtained access to the original, but find an abstract in the Archives de Phys. norm. et path. 1876, p. 175. See also Barker, p. 12, *op. cit. suprà.*

were accompanied by a fall of the curve, which he regarded as proof that more blood goes to the brain and less to the arm during emotion, or mental action, than at other times. But the following year these observations were repeated with great care, and with an improved plethysmograph, by Basch, of Vienna*, who failed to verify them. Most of the phlegmatic Germans on whom he experimented did sums in their heads, and otherwise exerted their minds, without producing the slightest modification of the curve; and none of them appear to have been as emotional as Dr. Pagliani, of whom Mosso relates that, his arm being in the plethysmograph, when the revered Prof. Ludwig entered the room the curve fell as if he had received an electric shock. Basch has cautiously investigated the causes of the varying quantity of blood in the arm in these experiments, and has clearly shown how many general and local conditions concur in producing the result. Especially has he emphasized the effect of variations in the abdominal circulation, which appear to exercise a much more considerable influence upon the size of the arm than any changes that occur in the brain.

In subsequent works Mosso has stated that during mental effort, such, for example, as is required to multiply small numbers in the head, the radial pulse, as recorded by the sphygmograph, is shown to become somewhat more frequent, and the recording lever does not rise so high as at other times†. Thanhoffer, who has pointed out that in these observations the influence of respiration on the pulse was neglected, concluded, nevertheless, from his own sphygmographic observations, that after due allowance is made for this complicating influence, it must be conceded that cerebral activity does exercise a certain effect upon the pulse, and in the direction stated‡. Eugène Gley, in a recently published essay, claims to have obtained similar results, and states that at the same time the sphygmographic trace of the carotid artery shows a higher upstroke of the recording lever, and other indications of dilatation of the vessel§. While these observa-

* Basch, "Die volumetrische Bestimmung des Blutdrucks am Menschen," Stricker's Med. Jahrb. 1876, S. 431. See also Rollet, in Hermann's Handb. der Phys. Bd. iv. Th. 1 (Leipsic, 1880), S. 306.

† Mosso, "Die Diagnostic des Pulses in Bezug auf die localen Veränderungen desselben," Leipsic, 1879; also by the same, "Sulla circolazione del sangue nel cervello dell' uomo," Rome, 1880.

‡ Thanhoffer, "Der Einfluss der Gehirnthätigkeit auf den Puls," Pflüger's Archiv, Bd. xix. 1879, S. 254.

§ Eugène Gley, "Essai critique sur les conditions physiologiques de la pensée. État du pouls carotidien pendant le travail intellectuel," Archives de Phys. norm. et path., Sept.-Oct. 1881, p. 741.

tions are not sufficiently numerous, or free from objections, to be accepted without question as proof that an increased supply of blood to the brain invariably accompanies mental effort, they are certainly sufficient to encourage further labour in this interesting field.

But if the arguments in favour of the purely material nature of our mental operations that have been based upon the imperfect results of the three lines of investigation I have just referred to must be rejected as utterly fallacious, what shall we say of the logic that attempts to draw a similar conclusion from the results of those inquiries into the phenomena of personal equation which aim at determining the time that must be allowed for the mental operation involved? * Do we, then, indeed need the beautiful experiments of Hirsch and Donders† to prove that thought occupies time? Whence, indeed, do we derive our primitive conceptions of time save from our consciousness of the succession of thought? And how could even the shortest time be occupied by even an infinite number of thoughts if each thought did not occupy at least some time, however brief?

I have thus, gentlemen, attempted to show that we are logically compelled to invoke the existence of a vital principle in order to account for certain important groups of phenomena occurring in living beings which cannot possibly be explained by the chemical and physical forces of the universe. These phenomena form a series, at one end of which we find the mere irritability or sensibility of the humblest mass of living protoplasm; at the other the reasoning faculty of the human mind. From the one extreme of this series to the other I recognize the manifestations of the vital principle. I willingly confess that I know nothing of the ultimate nature of this principle, except that it must be very different from the chemical and physical forces whose operations I have learned to recognize in the organic as well as in the inorganic world; nevertheless I am compelled by my study of the phenomena to conclude that it exists. I know that Mr. Huxley, only last summer, declared in the International Medical Congress at London, that the doctrine of a vital principle is the "asylum ignorantiæ of physiologists"‡; but this ancient sarcasm has now been applied to so many things that it has long since lost whatever sting it may once have possessed, when it was fresh

* Barker, p. 11, *op. cit. suprà.*

† Hirsch, "Détermination télégraphique de la différence de longitude entre les observatoires de Genève et de Neuchâtel," Genève et Bâle, 1864. Donders, in Reichert and Du Bois-Reymond's Archiv, 1868, p. 657.

‡ T. H. Huxley, "The Connexion of the Biological Sciences with Medicine," 'The Popular Science Monthly,' October 1881, p. 800.

and new. And I also know that one of the chief characteristics of true science is the sharpness with which it enables us to discriminate between that which we have proven and really know and that which we have not proven and do not know. Better far is it, and a thousand times more in accord with the simple honesty of science, to acknowledge frankly the truth, that phenomena occur in living beings which the inorganic forces do not explain, than to mistake our wishes for discoveries, to convert conjectures into dogmas, or, worst of all, to transform an undemonstrated hypothesis into a superstitious, aggressive, and intolerant creed.

Nor will the soundness of the conclusions at which the present generation shall arrive as to this matter, be without its practical effect upon methods of biological research, and the consequent future progress of biological science. It is not a mere metaphysical subtlety, but a subject of practical importance, that I have asked you to consider to-night. For if the chemico-physical hypothesis of life be true, the only road of progress in biology lies through the chemical and physical laboratories. Now, I have already this evening more than once indicated how highly I esteem the class of biological work that has already been done in these laboratories, and I have endeavoured to show how large is the unexplored biological field that can be explored only in this manner. But in addition to all that we can ever hope to do in this direction—and I insist upon its importance—I insist also upon the importance of other lines of work: I insist upon the importance of the systematic study of the phenomena of growth and development, of generation and heredity, of sensibility and mind. All that can thus be learned we need to know, and not merely for its own sake. This knowledge is indispensable to the right interpretation of the succession of life upon the globe in the past, and the successful direction of the interference of the human will with the future succession of life upon the globe in accordance with human necessities. We shall make slow progress in this direction if we confine our efforts to the application of chemistry and physics to those phenomena of living beings that can be thus explained. The other phenomena, not thus explicable, must also be studied in detail, arranged into orderly groups, and made the basis of such inductions as our knowledge of them may warrant. It is only by pursuing this method that we can hope ultimately to acquire, with regard to the phenomena of living beings, that power to predict, which is the criterion of true science, and that power to control, which we so sorely need.